

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIII.

March 14, 1873.

No. 5.

PROFESSOR CAYLEY, President, in the Chair.

J. M. Landsborough, Esq., Bradford, and
E. B. Knobel, Esq., Burton-on-Trent,

were balloted for and duly elected Fellows of the Society.

Copy of a Letter from the Astronomer Royal to the Secretary of the Admiralty expressing his Views on certain Articles which had appeared in the Public Newspapers in regard to the approaching Transit of Venus.

(Communicated by the Lords Commissioners of the Admiralty, and printed by order of the Council.)

I have the honour to acknowledge your letter of February 14th, calling my attention to articles which have appeared in the *Spectator* newspaper of February 8th, and the *Times* of February 13th, and requesting my views on these for the information of the Lords Commissioners of the Admiralty. I have procured the papers in question, and also the *Times* of February 20th, which contains articles of opposite characters bearing on the same subject; and have also received a paper by Dr. Oppolzer of Vienna, of very elaborate character, scarcely known in this country. I have the honour now to offer my remarks upon the whole subject.

2. The English papers are moderate and courteous in character, though distinct in their meaning. They are based entirely

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upon investigations by Mr. R. A. Proctor published in the *Monthly Notices*, especially in the *Notice* of June 11th, 1869. These investigations are illustrated by tables and maps of great clearness and unquestionable value; and to these, mainly, I shall refer in the following remarks. I shall, however, also extract some numbers from the calculations made by the Superintendent of the *Nautical Almanac*, and published in the *Nautical Almanac* for 1874; and some notes from the paper of Dr. Oppolzer to which I have alluded.

3. Remarking that the proposal to make observations on the Antarctic Continent is essentially connected (as I shall shortly show) with the idea that efficient observations may be made in the extreme north of Asia, I will first point out that this matter entered into my consideration in my original communication to the Royal Astronomical Society in 1857. In speaking of "the application of the method of difference of duration of transit" (called "Halley's Method" in the newspapers) "to the transit of 1874," I said, "The most northerly stations are to be found in Siberia, Tartary, and Thibet (which will scarcely be visited by Astronomers in December), on the coasts of China, and in North British India." And, remarking that the whole discussion is founded on the assumption by Mr. Proctor that comparisons of duration of transit at different places possess a high value, I state that in 1869 M. Puiseux had indicated this; that in the *Monthly Notices*, March 12th, 1869, I answered M. Puiseux, showing that I had already considered his suggested stations, and also that a ratio of probable errors to which he had not adverted must be taken into consideration. I believe that the idea has not again been promulgated on the Continent. I advert to these historical points for the purpose of showing that the matter has been sufficiently present to my mind. Subsequently Mr. Proctor took up the question, straining to the utmost the idea of separating as far as possible the northern and southern stations, and considering nothing but their geometrical relation.

4. I may here conveniently cite some passages (translated) from Dr. Oppolzer's paper, page 75: "In Halley's method in general, the longitude of the place of observation needs only approximately to be known, in order to determine the value of the parallax from the time of duration; and this was in the eighteenth century to be regarded as an especially great advantage, as the accurate determination of longitudes was then an almost insoluble problem; but in the present very perfect knowledge of the Moon's motion, there will be no very great difficulty in obtaining satisfactory determinations of the longitude. The application of Halley's method possesses therefore in the present day no special advantage. If the longitude is determined with certainty, it is of more advantage to the accuracy of the result not to use the observations as durations, but to treat them as made for the application of Delisle's method [the comparison of absolute times at different stations]."

5. My own preparations have been going on entirely in the same spirit as that which influenced Dr. Oppolzer in writing these remarks. The labour which I contemplate as to be employed in determining the local longitudes very far exceeds that for the mere observation of the transit. In a stay of three months I hope to be able to obtain at each station 30 meridional transits of the Moon, and 120 extrameridional transits, vertical or horizontal; and I do not doubt that the longitude obtained will be certain within 1^s of time.

6. Assuming (as Mr. Proctor appears to have assumed, and with my assent thereto) the probable error of clock observation of an ingress or egress as 4^s.28, then the probable error of absolute time in seconds is $\sqrt{\{(4.28)^2 + 1\}}$,* and the probable error in the comparison of absolute times at two stations will be $\sqrt{2} \times \sqrt{\{(4.28)^2 + 1\}}$. This is the probable error for Delisle's method. When both ingress and egress are observed at one station, the probable error of each is 4.28, the probable error of the interval is $\sqrt{2} \times 4.28$, and the probable error of the comparison of the intervals thus observed at two stations is 2×4.28 . This is the probable error for Halley's method. The proportion or $\frac{\text{Halley's}}{\text{Delisle's}}$ is 1.379. The demerit of a comparison of observations depends on the proportion

$$\frac{\text{Probable Error}}{\text{Difference attributable to Parallax}}$$

Therefore, in order to ascertain whether a comparison in Halley's method is as good as one in Delisle's method, we must find whether the parallax-difference in Halley's method is greater than that in Delisle's method in the proportion of 1 : 1.379. And taking Mr. Proctor's selection of a fundamental Delisle's comparison (Woahoo—Crozet Island) at 23^m.8, and remarking that $23.8 \times 1.379 = 32.8$, we finally arrive at this simple criterion, that if the parallactic difference of duration in Halley's method in any special comparison of observations is greater than 32^m.8, that comparison is more valuable than one in Delisle's method; if less than 32^m.8, it is less valuable than one in Delisle's method.

7. I will now exhibit the application of this criterion to three northern and three southern stations :—

Station.	Parallactic Effect.	Comparison with Enderby.	Comparison with Nertschinsk.
	m	m	m
Nertschinsk	15.6	35.9	
Tientsin	13.4	33.7	
Pekin	12.9	33.2	

* In Mr. Proctor's formula, *Monthly Notices*, 1869, June 11, page 315, the factor $\frac{1}{2}$ is omitted by a printer's error. The computed numbers appear to be correct.

Station.	Parallactic Effect.	Comparison with Enderby.	Comparison with Nertschinsk.
	m	m	m
Enderby	20.3	..	35.9
Crozet	16.8	..	32.4
Kerguelen	16.6	..	32.2

The combination of Nertschinsk and Enderby gives a number greater than the criterion ; and therefore, if there were no other difficulties in the way, it would be well to combine these two stations. But the combination of Nertschinsk with Crozet or Kerguelen gives a number below the criterion ; and therefore, if Enderby is not secured, the observation at Nertschinsk possesses no special value of the kind considered. The combination of Enderby with Tientsin or Peking gives a number scarcely above the criterion, and therefore, if Nertschinsk is not secured, the observation at Enderby scarcely possesses any special value.

8. I will now advert to the local circumstances. A few of the following numbers are taken roughly from Mr. Proctor's maps, but none are seriously in error.

Station.	Latitude.	Sun's Elevation a Ingress and Egress	
	°	°	°
Nertschinsk	51 +	12	10
Tientsin	39	23	22
Peking	40	22	21
Enderby	(66)	(20)	(40)
Crozet	(46)	(11)	(50)
Kerguelen	49	28	59

Nertschinsk is a station in Siberia, in high latitude, nearly 1000 miles from the nearest sea. I presume that its climate is truly continental. At St. Petersburg, in the winter, the Sun sometimes is not seen for several weeks together; I suppose that the same may happen at Nertschinsk. The Sun's elevation at the observations will be rather small. I doubt greatly the probability that any observations can be made there.

Of Enderby's Land very little is known. It is certain that an expedition to that region must be exposed to long confinement and great severities, affecting even the metallic instruments.

9. On a review of the whole case, I decline to recommend that an expedition be sent to Enderby Land, or to any station in the Antarctic Continent.

10. In one of the papers to which my attention was called, it was suggested that, referring to the belief that Lord Lindsay proposes to make observations at the Mauritius, the Rodriguez observations should be abandoned, and a ship thus set at liberty for the South Continent. I very highly respect Lord Lindsay's spirit of enterprise, but there are abundant reasons for refusing to abandon a Government expedition in the hope that a private

Astronomer will do something equivalent. I add that the position of Mauritius is inferior to that of Rodriguez.

11. It seems to be conceived by the writers of the papers that no attention has been given to India. For myself I can say that it has been duly considered, but that, in reference to the original proposal of relying on eye-observations, I saw no reason for establishing a station there, and in particular I objected to a proposed substitution of Peshawur for Alexandria. The introduction of photography has introduced new geometrical considerations, principally pointed out by Mr. Proctor; and I have endeavoured to carry out the establishment of a photographic station in India. The matter is now in a stage in which I have no control or official information.

12. The only extension of the original plan to which I look as a contingency is this. My scheme was drawn up, and action was taken on it, before the outbreak of the war between France and Germany. In proposing that Great Britain should take up a station in the Sandwich Islands, I expressly recorded my hope that France would equip a station at the Marquesas. This hope may now be frustrated, although I know so well the noble spirit of that nation in all matters relating to science that I can scarcely entertain the thought. But I am not aware that any positive arrangement has yet been made, either by the French or the American Governments, for filling up the gap in the Pacific. At present, that is the weak part of established plans. Should the deficiency continue to exist, some effort might well be made to supply it.

Royal Observatory, Greenwich,
1873, February 21.

Remarks on a Letter from the Astronomer Royal to the Secretary of the Admiralty, on the subject of the approaching Transits of Venus. By Richard A. Proctor, B.A., Cambridge.

I take the articles of this letter *seriatim*.

1 and 2 need no comment other than a recognition on my part of the courteous tone in which they are expressed.

3. I shall presently show that efficient observations at high northern stations are not necessary in order to render observations by Halley's method superior to the proposed observations by Delisle's. I am at a loss to understand how the question can be said to have been sufficiently taken into consideration in the Astronomer Royal's original communication to the Royal Astronomical Society in 1857. I would refer to that communication itself, and to my remarks upon it and quotations from it elsewhere in the present Number of the *Notices*. The mere circumstance that the estimated epochs of ingress and egress were nearly an hour in error would suffice of itself to show that that communica-